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EXPERIMENTS UPON PHYSIOLOGICAL MEMORY BY MEANS OF THE INTERFERENCE OF ASSOCIATIONS.

BY JOHN A. BERGSTRÖM.

The interference of associations is a fundamental fact of the nervous system and so of frequent occurrence. In learning a new way of playing an instrument, or a second system of shorthand, or a new style of writing, or in substituting a new movement in gymnastics, or any game of skill; in general, in changing from an accustomed way of doing a thing to a new way, the old habit resists displacement; and if we act hastily, or are fatigued or absorbed in something else, we are liable to act in the old way. It is not evident, however, without experiment that the old habit interferes with the formation of the new one. It is easier to act in the old way, but it is not evident that we cannot learn a new way, if we try, as easily as we learned the old one. The reason we follow the old way may be a disinclination to effort. In the small field of the present experiments it will appear, however, that in spite of every effort a very decided interference takes place when we attempt to associate a new reaction with an old stimulus, and that the interference is so constant that it may be made a means of measuring memory just as the greater ease in re-learning something once learned has been used in other experiments. The confusion which results in the minds of young students in presenting a subject to them in more than one way is an illustration in another field. Much that has been attributed to the decreasing of memory power with age, as in the case of learning languages, should, perhaps, come under the category of interference. Interference, as well as practice, should be taken into account in all psychological experiments when the same stimuli or signs are used in different relations.

Methods and First Experiments. The method to be described was developed in studying the errors of an experiment upon quite another problem. In this, use was made of a test based upon the method of finding distinction and choice

times by means of an ordinary pack of playing cards.¹ Unprinted cards with the best slipping qualities were procured and made up into packs of 80 each. There were ten kinds of cards in each pack, each kind marked by an abstract word printed at the top. The cards were well shuffled and sorted as rapidly as possible into ten piles, each pile having all the cards with a given word. Chance determined the order of the piles, and this was in the main different in successive experiments. Each experiment consisted of sorting in quick succession two packs bearing the same words into different piles, which we may represent by letters as follows:

1.					2.				
<i>A</i>	<i>B</i>	<i>C</i>	<i>D</i>	<i>E</i>	<i>G</i>	<i>J</i>	<i>H</i>	<i>A</i>	<i>F</i>
<i>F</i>	<i>G</i>	<i>H</i>	<i>I</i>	<i>J</i>	<i>I</i>	<i>C</i>	<i>E</i>	<i>B</i>	<i>D</i>

In sorting the second pack the time was longer and the subject appeared confused, and had a tendency to place the cards in the positions they had had in the first half of the experiment. Two different reactions were associated with the same sensory stimulus and interference took place. The experiment was not directed to the study of this phenomenon, and the indirect results we obtained simply established the fact of interference. During the last of March and the first of April, '92, eight to ten experiments a day were made at regular intervals for from three to eight days in the case of the following persons. The first column gives the average time in seconds for sorting the first pack, and the second the excess of time for the second pack:

1.		2.	
W. L. B.,	158.7		17.5
L. L. B.,	122.5		10.1
A. F.,	127.8		18.
E. N. B.,	158.		16.8
W. O. K.,	145.9		3.5

When the second pack bore different words from the first there was practically no interference, as appears in the results for another subject, F. B. D.

The average of nine experiments on nine different days upon F. B. D. gave a difference of 30.25 seconds when the first and second packs were alike; and for three days in which two packs with different words were sorted successively, a difference of only 0.53 seconds. The pack with different words was procured to avoid the interference due to mak-

¹Science, VIII., 1886, 238.

ing the first associations. If the words were different in the two packs and the retardation took place nevertheless, a part of it might be supposed to be due to fatigue. If no lengthening of time occurred, then the differences in the experiments with the packs having the same words would all be due to the interference of the associations. The decision of this point, however, must be left to the direct experiments presently to be described, since the evidence from these early indirect ones is incomplete. In 23 and 30 experiments respectively with A. F. and W. O. K., a second pack, different from the first, was sorted more quickly by 2.7 and 3.1 seconds. A part of this difference may be due to a difference in the packs, since, unfortunately, the order in which they were taken was not changed to make compensation.

Later Experiments. In order that the interference of associations might be under favorable experimental conditions, the sensory and mechanical parts of the process were simplified as much as possible. Instead of words, pictures of common objects were drawn at the top of the cards, and care was taken to have every picture different from every other. One set, for example, consisted of a house, a book, a coffee-pot, an acorn, a palette, a clothes brush, a glass, a safety-pin, a fan, and an oak leaf. They must be easily distinguished and not liable to grouping or other mnemonical devices; and all the pictures in all the sets must be different. The cards were held face up, and thrown upon a table covered with a rough white cloth to keep them from sliding.

The first problem studied was the rate of decrease of the interference with increasing intervals of time between the first and second sortings. An experiment consisted of two parts: the first, the sorting of a pack of cards into piles in given positions; the second, the sorting, after a certain interval of time counted from the end of the first to the beginning of the second parts, of a pack having the same pictures into piles in entirely different positions. Seven, and in one case eight, different intervals were used ranging from three to 960 seconds. One experiment a day for each interval was taken at a given hour. This required eight double sets of cards, the actual number of which was reduced one-half by having pictures at both ends of the cards. A complete record of the positions occupied by the cards from day to day was kept so that no repetitions should occur till after several days. Since a card can occupy any one of the nine other positions than the one in which it first occurs, there are ten arrangements possible without a single repetition. It would probably make the results more regular to have a sufficient number of sets to make any repetition whatever unnecessary. The relative

results sought in this case should, however, be nearly the same, since all the packs received approximately the same treatment, though we may expect to find the probable errors made somewhat larger thereby. Two minutes' rest was allowed between the experiments on different intervals, and all together they required nearly an hour a day. To allow for practice, fatigue and changing conditions in the hours, the experiments were begun with the different intervals in turn. The cards were usually picked up from the table irregularly, but sometimes shuffled, and in either case care was taken that no two cards with the same pictures should come together and that there should be no recurrence of orders in the packs. The results obtained appear in the following tables:

Table I. Subject, F. D.; member of the Pedagogical Department; unpracticed in this kind of experimental work; not aware of the purpose of the experiment till the end; refrained from any speculations about the matter. In sorting the first pack, the ability to throw the cards without looking at the piles was reached in some cases; usually all but two or three positions were known. There was no tendency to employ mnemonical schemes. In the second sorting, false movements were very numerous at first; some tendency to employ mnemonics; so strong a habit of looking towards the former positions that cards directly in front were often hard to recognize. The records for the first four days were only partial and very irregular, and so were not used in making out the average. The rest number 16 days, or 112 experiments. The first horizontal row of figures in all the tables gives the intervals between the first and second packs. The second and third horizontal rows of figures, with the exception of the first and last figures, give the corresponding difference between the sorting of the first and second packs, each figure being the average of eight days' records. Next comes the average of the differences, then the probable error of these averages. If we assume that at each interval the same quantity is measured, and that the variations are due to accidental errors, Chauvenet's criterion for the rejection of observations may be applied. The result is given in the last row. The first figures at the left (Av. P.) give the average of all the first packs for the time indicated, and so show the result of practice. The figures at the right (Av. D.) give the average of the differences, which, in this case, show a slight increase rather than decrease with practice. The condition of making the greatest possible haste was not strictly complied with, since the subject had a tendency to select the rate he thought would enable him to make the best time. All figures represent seconds. The averages of the differences from Table

I. are shown in graphic form in Fig. 1 of the diagram at the end of the paper.

TABLE I.

	(Av. P.')	3''	15''	30''	60''	120''	240''	480''	Av. D.
Oct. 25-Nov. 2.	80.52	26.2	23.3	17.93	17.16	20.03	14.4	13.06	18.87
Nov. 3-12.	72.62	27.03	19.88	23.45	25.	20.18	16.35	14.06	20.67
Average of Dif.		26.62	21.59	20.69	21.08	20.11	15.88	13.56	
P. E. of Av.		1.23	1.25	1.50	1.45	1.18	.96	.97	
Corrected by Chauv. Crit.		26.64	22.64	22.02	21.8	19.10	16.25	15.29	

Table II. Subject, O. C.; member of the Pedagogical Department; unpracticed in this kind of work, and also unacquainted with the purpose of the experiments. As in the first case the first four days are omitted in making up the

TABLE II.

	(Av. P.')	3''	15''	30''	60''	120''	240''	480''	Av. D.
Oct. 24-Nov. 2.	67.59	18.46	15.33	15.83	14.39	13.31	13.72	14.28	15.05
Nov. 3-12.	63.72	16.2	11.63	12.54	12.31	8.00	9.72	8.42	11.29
Average of Dif.		17.33	18.48	14.18	13.45	10.66	11.72	11.34	
P. E. of Av.		.91	.74	1.17	1.26	1.26	1.25	1.18	
Corrected Results.		17.39	13.48	12.42	12.2	9.14	10.2	9.27	

table. Eighteen days' records remain—126 experiments. The averages in the second and third horizontal rows are made on the basis of nine records for each. Here also the tendency to regulate the speed so as to make the best time

was noticed. The eyes were very active, following the throwing of each card, as far as the experimenter could judge. The nice arrangement of the piles would show the same. The differences are comparatively small, and as is seen in column (Av. D.), there is a considerable decrease in the average of the differences. The constant use of the eyes did not permit so strong a habit to be formed ; and their quick motions probably indicated an individual peculiarity of temperament and muscular control, though these may be dependent upon the absence of strong tendencies in the directions of the previous positions, which seems to the writer most probable. The averages of the differences from Table II. are shown in Fig. 2 of the diagram.

Table III. Subject, M. E. B. ; acquainted with this kind of work from acting as experimenter in the experiments of Table IV. ; also trained in sorting other cards than those used. All the records taken are made use of in the table, since there was no need of allowing for practice in the manipulation of the cards. By watching the eyes of the subject, the time when all the positions were learned could be noted down. The average of this time for the last three days is 34 seconds for the first pack ; the last was rarely learned entirely, even with the longer intervals, and then only after 65 or 70 seconds. The greatest possible speed was attempted, and there was no conscious adjustment of the speed to make in the judgment of the subject the quickest time. There are 12 days' experiments in all—94 records. The 960" interval has only 10 records. The averages in the second and third rows are based on six records each, except as just stated, those of 960" interval. The averages of the differences from Table III. give Fig. 3 of the diagram.

TABLE III.

	(Av. P.)	3"	15"	30"	60"	120"	240"	480"	960"	Av. D.
Nov. 8-13.	60.08	29.2	23.66	17.23	19.03	13.71	12.83	11.2	10.22	17.11
Nov. 14-20.	59.37	28.38	23.65	22.55	15.66	13.43	13.36	8.96	5.36	16.42
Average of Dif.		28.79	23.66	19.89	17.34	13.57	13.09	10.08	7.79	
P. E. of Av.		.70	.92	1.20	1.01	1.31	1.23	1.62	1.11	
Corrected Results.		28.79	23.66	21.16	18.42	12.26	13.09	10.08	7.79	

Table IV. Subject, the writer. During all but the first seven days of the experiment the subject was engaged two hours a day with experiments upon others. Every effort at speed was made, and there was no conscious allowance for what might be the best rate. The records were taken between 7 and 8 A. M. No records are omitted to allow for practice; but three days' records are omitted in the latter part of the time, because of some irregularities in health and sleep, which made the circumstances abnormal. The average, with these included, is given below for comparison. The table is based on 21 days' records—147 experiments. The averages of the differences from Table IV. give Fig. 4 of the diagram.

TABLE IV.

	(Av. P.)	3"	15"	30"	60"	120"	240"	480"	Av. D.
Oct. 13-20.	84.15	31.07	26.71	16.69	12.19	14.28	11.29	8.71	17.37
Oct. 21-31.	65.68	35.86	27.07	25.60	19.57	11.54	12.29	13.39	20.76
Nov. 1-9.	61.98	31.71	21.76	18.97	15.53	15.43	12.56	11.03	18.14
Average of Dif.		33.09	25.18	20.42	15.76	13.75	12.04	11.04	
P. E. of Av.		1.20	1.07	1.10	.83	.81	.65	1.04	
Corrected Results.		32.15	25.18	19.50	15.76	13.75	12.00	11.04	
Ave. of all 24 Records.		32.65	25.75	20.08	16.3	14.94	12.08	11.54	

Table V. These records, though taken first, are placed last, because the arrangement of the experiment is different. Two experiments were taken every two hours, between 8 A. M. and 6 P. M. The cards with words instead of pictures were used. The experiments were so distributed that two records at each time of the day were to be taken for each interval. It was hoped by this means to find not only the rate of disappearance of neural memory in general, but also the difference for different times of day. The plan of the experiment is open to objections on other grounds, but the fact that the same cards were used incessantly, and that only two hours

were allowed for the habit to decrease in intensity before another trial was made, would lead us to expect great irregularities. The subject was T. L. B.; familiar with psychological experiments, and in good practice with the cards; acquainted in a general way with the purpose of the work. To Table V. corresponds Fig. 5 of the diagram.

TABLE V.

	(Av. P.')	3"	15"	30"	60"	120"	240"	480"
June 1-13.	80.86	21.85	21.42	17.88	12.25	10.02	11.2	10.25
P. E. of Av.		2.28	2.19	1.87	2.14	2.02	3.96	1.44

There are 66 experiments in all; 10 or 11 experiments each for all, except the last two intervals, for which there are only 7 and 4 respectively.

These differences might, perhaps, be supposed to be due in large measure to fatigue. To test the effect of fatigue or practice, some experiments were made on successive days, as before, in which two packs with different pictures were sorted successively with the shortest interval, where fatigue would show itself if anywhere. Half the experiments began with one, half with the other pack, to compensate for any differences in them. The averages here as in the other experiments are in seconds. P^1 and P^2 are the first and second packs respectively. The first two averages include 12, the last two 16 experiments each.

TABLE VI.

	P^1	P^2	$P^1 - P^2$
O. C.	63.53	62.94	.59
F. D.	66.8	65.37	1.43
M. E. B.	56.62	56.49	.13
J. A. B.	64.61.	63.14	1.47
Average.	62.89	61.99	.90

Thus when there is no chance for interference to take place the tendency is in all cases to sort the second pack a little quicker than the first. We may conclude, therefore, that the differences are not due to fatigue.

The attitude of the subject is here the reverse of that in the work of previous experimenters. In their experiments, his interest was to remember, in these to forget. The influence of the knowledge that something is to be remembered upon its retention is well known. The relatively large amount of interference throughout the series, and the slight diminution of the average of the differences, as seen in the right-hand column (Av. D.), makes it probable that forgetting—that is, the disappearance of habit—is not an active process of disintegration, but merely a letting alone. The rapid rate of disappearance at first was one of the most striking facts noticed in the preliminary experiments.

The uncorrected average differences, and their average for all the subjects, are given together for the sake of comparison in Table VII.

TABLE VII.

	3"	15"	30"	60"	120"	240"	480"	960"
F. D.	26.62	21.59	20.69	21.08	20.11	15.88	13.56	
O. C.	17.33	13.48	14.18	13.45	10.66	11.72	11.34	
M. E. B.	28.79	23.66	19.89	17.34	13.57	13.09	10.08	7.79
J. A. B.	33.09	25.18	20.42	15.76	13.75	12.	11.04	
T. L. B.	21.85	21.42	17.88	12.25	10.02	11.2	10.25	
Average.	25.54	21.07	18.61	15.98	13.62	12.78	11.25	[7.79]

Reference to the results corrected by Chauvenet's criterion shows that those of F. D. and O. C. follow the average in general more nearly than the uncorrected results; but the assumption on which the corrections rest is at best only approximately true. The average difference between the 3-second and the 8-minute differences is 14.28 seconds; and between the 1-minute and the 8-minute differences 4.72, which

shows that about $\frac{2}{3}$ of the decrease took place in the first minute. Indeed, references to the tables will show that we can hardly be sure of any decrease from 2 to 8 minutes with the few observations we have. The heightened form of neural dispositions, due to immediate intense activity, drops quickly to a low level, from which the decrease is comparatively slow. The attention is in this case turned away completely from the first positions, since the subject's interest is to forget rather than remember them. In 24 hours the subject can sort the cards as rapidly as at first. This, however, does not mean that the neural disposition has vanished. The experiment consists rather in raising a habit temporarily above the rest, and noting its interference with another raised similarly above the same base of opposition. The difference at 3 seconds does not measure the strength of the habit at 3 seconds from the time of sorting the first pack, but is relatively too small, since the habit is found to decrease rapidly during the process of sorting the second pack. A considerably weaker habit applied uniformly as an interference might have produced the difference. Only an estimate can be made as to how many seconds of work with the second pack would have had to elapse before this average habit would have been reached. If we put it at 30 for the 3-second interval, it would be nearly 45 for the 15-second interval; and the decrease in the differences would be between these two points. But after about a minute and a half, when the great decrease in the differences is at an end, the method measures accurately the rate of forgetting.

A tentative analysis of the process may be permitted here, as it will bring out some of the essential conditions of the experiment. In sorting the first pack, two of the subjects learned the positions during the placing of about a third of the eighty cards, so that the process could go on without further use of the eye to find the place for, or to verify, each throw. After a little practice there was very little feeling of effort, and the process was largely mechanical. It takes on the average all the subjects when in practice 65 seconds to sort the first pack; to sort the second immediately afterwards requires, however, about 85 seconds, and numerous false motions in the direction of the first positions are made. The false movements, and the errors which the subject was obliged to correct, and the consequent retardation, show that a strong association has been formed. At first the eye is moved in the direction any card is to occupy. The movement of the arm in placing the card may follow the eye as a reflex or as a separate act of the will. In the first case, in which we have supposed the arm to follow the movement of the eye reflexly, the association which

interferes would be between the visual centres receiving the image of the picture and the centres moving the eye. As soon as the picture was seen, the eye would snap in the direction of the strongest habit, or toward the former positions, and the arm would follow and make the visible false motion, not because of an association between its centres and the visual centre, but because it is in reflex *rapport* with the eye. In the second case, the association would be formed between the image of the picture and the arm movement, and the eye movement might be unimportant in the production of the interference; or the picture may be the signal for sending an impulse both to the eye and the arm at the same time. The localization of the interference is an important psychological question. It is a mechanical struggle of habits, and might at first be supposed to be very definitely localized, so that the association of the positions with the motor organs of speech would not interfere with sorting by the hand. In nine experiments, in which the positions were learned from seeing the cards on the table by repetitions, like those employed by Ebbinghaus with the nonsense syllables, an interference of 30.02 seconds took place, as compared with sorting a pack, without such learning, immediately before and after each series of experiments. In another series the positions were learned, not by looking at the cards, but by being told where each one was, till they were learned. It was supposed that by using neither the eye nor the arm, but getting the information by another sense, and expressing it with the vocal organs, interference might be avoided for the sorting when the eye and arm were the organs used. The interference in this case was 23.25 seconds, as the average of nine experiments. The positions were learned in each case, till they could be repeated forwards and backwards. The interference is considerably less when the ear was the receiving sense, but the experiments are too few to make definite comparisons. The whole matter requires additional experimental study. As far as these experiments go, they show that it makes little difference what sense is the receiving sense, or what organ the reacting organ, as regards the production of interference for the sorting by hand. The information gained by one sense is perhaps not confined to the centres of that sense, but awakens the other senses whose special data have contributed, or may contribute, to the perception of the object—the eye visualizing, the arm moving, or tending to move, as if it were the organ used.

In sorting the cards quickly, the mental process is not an intense form of the process that would take place if the sorting were done slowly. In the latter case no false motions

need be made, since the subject would know that no position of the first stage of the experiment was repeated in the second, and if he made a mistake, he would learn from experience, and not repeat it. Only a very small part of such complete consciousness is present in rapid sorting, since not only are errors made once, but the same error is repeated many times, especially with the shorter intervals. When the effort is to make the greatest possible speed, we produce the condition of automatism voluntarily. We have in this a laboratory method of studying habits.

An illustration of interference is found in the familiar game in which a person is asked to repeat sentences like the following as fast as he can: "Shall he sell sea shells? Shall she sell sea shells?" "Shoes and socks shock Susan," or, "Peter Piper picked a peck of pickled peppers," etc. Great difficulty is experienced. In the first half of the first sentence the simple *s* sound is repeated twice in "sell" and "sea;" it begins "shells," and if the person repeats very rapidly, so as to exclude the higher mental processes, the habit formed by simply repeating the *s* sound twice will make him say "sells" for "shells." Similarly the repetition of *sh* twice in the second half will probably make him say, "shall she shell." Every one is familiar with similar errors in writing, where words beginning with the same letters are liable to confusion. This is especially noticeable in fatigue, or cases of abstraction and haste. The cards provide an easy means of variations, so that the phenomena can be studied quantitatively. After considerable practice, the visible number of false moves diminishes, but as will be seen in the tables, the average retardation does not grow very much less. A habit of hesitation has been formed, but does not lessen the interference very much, as measured by the increase in time.

Historical.

An extended account of the experiments of others on memory is not needed here, Dr. Burnham's study in the second volume of this *Journal* being sufficiently accessible. A few papers, however, have been published since Dr. Burnham wrote,¹ and some of them have more or less relation to the experiments of this paper.

¹Paneth: *Versuche über den Zeitlichen Verlauf des Gedächtnissbildes*. Centralblatt f. Physiol., Bd. IV., No. 3, 1890.

Schumann: *Ueber das Gedächtniss für Komplexe regelmässig aufeinander folgender, gleicher Schalleindrücke*. Zeitschr. f. Psychol., Bd. I., 1890.

Schumann: *Ueber Contrasterscheinungen in Folge von Einstel-*

Müller and Schumann give an account of some interesting and important observations on what they call motor adjustments. If a heavy weight is lifted a number of times, a habit of expending a given amount of energy is formed, and lighter weights lifted afterwards will appear lighter than they ordinarily do. The method of experimenting is first to find a comparison weight heavier than that which can just be distinguished from the standard each time ; then the heavy weight is lifted in place of the comparison weight several times. On returning to the comparison weight, it can no longer be distinguished, and may even be judged lighter than the standard. The influence of the motor adjustment was noticed even after twenty-four hours. The phenomenon is thought to be due, not to contrast, but to the formation of the motor habit, and the illusion takes place because the comparison weight is lifted with greater velocity than usual. Dr. Schumann has given an account of similar phenomena in another field. The normal rate of letters on a revolving drum seemed slower or faster than usual, after a short habituation to faster or slower velocities respectively. Vigor and fatigue seem to have a somewhat similar effect. Distances traced by the hand were misjudged after habituation to other distances. These phenomena, which resemble the "interference" of this paper, are very important for psycho-physical experiments as well as for a study of the nervous system.

Dr. Münsterberg has studied the interesting theoretical question whether a habit associated with a given sensory stimulus can continue automatically, while some effect of a previous and different habit associated with the same stimulus remains. He believes the question can only be studied when the attention is entirely distracted from the experiments, and therefore it is to be studied in the actions of daily life, not in the laboratory. The experiments were made with his inkstand, his watch, and the doors of his room. In each case a given habit is practiced (in the course of daily life—not specially) till it becomes automatic—as, for example, taking his watch out of his pocket on the left side. A different habit is then practiced ; in the case of the watch, of taking it out of the

lung. Nachrichten v. d. k. Ges. d. Wiss. zu Göttingen, Dec. 3, 1889.
No. 20.

Müller and Schumann: Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte. *Pflüger's Archiv*, Bd. XLV., 1889, pp. 37-112.

Münsterberg: Gedächtnisstudien. *Beiträge*, Heft 4, 1892.

Cattell and Fullerton: On the Perception of Small Differences. *Publications of the Univ. of Penna. Philos. Series*, No. 2, 1892, pp. 147-149.

pocket on the right side till this is done automatically. He then returns to the old habit, and finds that it takes less time to relearn this than it did to learn the second, and therefore concludes that some effect of it remained, although the second was automatic. If this process is repeated several times with the same object, the time required for relearning each of the two habits grows less and less. This shows, he believes, that the habits are gradually being developed to their maximum strength, and that only a little difference one way or the other is sufficient to make the habit do its work automatically, and that nerve currents do not behave like electric currents, which divide into several conductors inversely as the resistance.

It only remains for me to express my obligation to those that have assisted me as subjects, and in particular to my wife, whose work both as a subject and assistant has contributed much to the success of the research.